

***Interactive comment on “The size resolved cloud condensation nuclei (CCN) activity and its prediction based on aerosol hygroscopicity and composition in the Pearl Delta River (PRD) Region during wintertime 2014” by Mingfu Cai et al.***

**Summary:**

This work demonstrates the field measurement results of CCN activity, hygroscopicity and chemical compositions of aerosol particles in the PRD region. The manuscript fits well to the scope of ACP and presents valuable results. Thus I recommend it to be published after the following comments listed below have been adequately addressed.

**Comments:**

1. Section 2.2.2: Please give more information of reference data used in the köhler theory when performing the CCNC calibration. This is very important because different parameterizations will retrieve different critical supersaturations (Rose et al., 2008; Wang et al., 2017).
2. Section 2.3.4: It should be section 2.3.3. Still, I am confused with the method to perform the CCN prediction based on HTDMA data. I would suggest the authors give an exemplary case either in the text or supplement referring to Lukas et al., (2010).
3. Table 1: where these data come from, please add reference. I guess these kappa data are retrieved with T of 298.15 K. But in Section 2.3.1, the T you used is 293 K, why?
4. Table 2: Based on the SMCA measurements, you should get size-resolved activation ratio, so I do not understand the max. and min. values of AR here? In principle, it should be 1 and 0. I guess you calculate the overall AR, please clarify and explain the reason why you put it here.
5. Page 16, line 8-10: Please explain this sentence, it is not clear.
6. Page 17, line 3-4: This is not consistent with the statement in the abstract, please revise.
7. Page 17, line 5-7: The difference between kappa-CCN and kappa-HTDMA may also be due to the parameterizations used in the CCNC and HTDMA calibration. See Wang et al., (2017). Please consider it and give more information as suggested

in comment 1.

8. Page 17, line 11-12: Any evidence? I guess the larger hygroscopicity is mainly due to the bigger particle size.
9. Figure 4, have you corrected the double charge effect of DMA?
10. Page 17, line 16-19: This sentence (“the peak in the less-hygroscopic mode declined ... while the one in the more-hygroscopic mode climbed...”) is not clear. Do you mean the relative fraction of less-hygroscopic compounds decreased and more-hygroscopic compounds increased at larger particle size?
11. Figure 7: I am not quite sure that the impacts of organics can fully explain the difference between the calculated and measured AR. The bias is still obvious even the configured surface tension (0.072) is used, indicating the other factors should also be considered. Many studies (Petters et al., 2009; Wex et al., 2009; Hersey et al., 2013; Wu et al., 2013; Hong et al., 2014; Hansen et al., 2015; Mikhailov et al., 2015; Pajunoja et al., 2015; Zhao et al., 2016) have reported the different hygroscopic properties from CCNC and HTDMA measurements. I would suggest more discussions should be added here.
12. Page 20, line 20-21: Add reference.
13. Figure 9: please provide  $R^2$ .
14. Figure 10 and 11: what does the black line mean? Is it 1:1 line? then the scale should be checked.
15. There are several grammar mistakes in the text, the language and symbols should be checked carefully once more before publication.

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